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NOTES AND LITERATURE

BIOMETRICS

Some Recent Studies on Growth.—The problems presented by the phenomena of growth change are not only peculiarly adapted to quantitative treatment, but perhaps more obviously and clearly demand the application of quantitative methods for their solution than does any other single large class of biological problems. In the study of variation and heredity it is an open question as to what relative importance is to be assigned to quantitative as compared with qualitative differences between organisms. But however much interest or significance qualitative changes occurring in connection with the growth process may have, it yet remains an indisputable fact that the fundamental and essential feature of the process is a quantitative change. While this has, of course, always been recognized by students of the subject, there still is to be seen evidence of the influence of the modern biometric standpoint in recent studies in this field. This is chiefly apparent in the increasing attention paid to precision and refinement in the mathematical methods used in the analysis of the distinctively quantitative phases of the problems of growth.

The most recent contribution in the series of memoirs by Professor H. H. Donaldson[1] and his students dealing with various phases of the problem of growth in the white rat is in some respects to be regarded as the most fundamental which has yet appeared. This paper gives in detail the basic data regarding the growth of the body as a whole and of the central nervous system in the white rat which have been collected in the course of a very extensive and thorough investigation. These data are given in a "general table" occupying thirteen pages and comprise records for 458 male and 215 female normal white rats. For each of these animals (with the few omissions of single measurements in scattered individuals unavoidable in so large a piece of work) there are recorded the following data: Series number, sex, age in days, body weight, brain weight, and spinal cord weight, each in grams. That the utmost care was taken to ensure the accuracy of these weight records really does not need saying in an American biological journal. The body weights are recorded to a tenth of a gram, and the brain and cord weights to a ten-thousandth of a gram. These unique data, involving thirteen years in the collecting, constitute a scientific achievement of much significance, not alone because of the intrinsic importance of the records for the study of growth problems, but also because they are a monumental example of biological data collected with physico-chemical exactness. The paper will stand as a classic in the literature on growth.

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[1] Donaldson, H. H. A Comparison of the Albino Rat with Man in Respect to the Growth of the Brain and of the Spinal Cord. *Journ. of Comp. Neurol. and Psychol.*, Vol. XVIII, pp. 345–392, Plates II and III, 1908.

The first portion of the paper deals with the growth of the rat's brain. The brain-weight data are plotted to a base line of body weight instead of to a base line of age and when so arranged are graduated with a curve of the general type.

$$y = A + C \log (x + \beta)$$

where y denotes brain weight, x body weight and A, C and β are constants.

The actual theoretical curve for the brain weight of the white rat is as follows:

$$y = .569 \log (x - 8.7) + .554.$$

This curve gives a very excellent graduation of the observational data. In fact, a closer agreement between theory and observation could not reasonably be expected. It is of some interest to note that this curve which describes the growth of the rat's brain in weight is of the same general type which has been found by Pearson and by the present writer to describe growth changes in various organisms.[2] It is all the time becoming more evident that this type of curve is a very useful one for growth work. Experience is showing that it undoubtedly has a wide range of applicability in describing the quantitative changes occurring in growth and various sorts of regulatory phenomena. While this fact is empirically obvious, no ulterior biological significance is to be attached to it. The biological significance of this fact appears to the present writer to be of the same *kind* as would attach to the discovery that some particular stain was useful for differentiating a wide range of cell

[2] Cf., for example, the data regarding growth in the plant Ceratophyllum presented in Carnegie Institution Publication No. 58.

structures, not before known to have anything in common. Such a result *might* mean that these structures all had a common cause or mode of origin, but to draw such a conclusion in the absence of confirmatory evidence of another kind than that afforded by the stain would be an exceedingly hazardous proceeding.

Donaldson points out that while the logarithmic curve describes very well the growth of the brain for the whole period from birth to maturity, the simpler relation proposed by Dubois, according to which the brain weight increases as some simple proportion (here the seventh root) of the body weight, fails to do this, since it holds only for the later growth period of the rat's brain. It fails entirely to graduate the data during the period of rapid growth.

The second portion of the paper deals with the growth of the spinal cord. This again is found to follow a logarithmic curve of the same general type as that which graduates the brain growth data though, of course, with different values of the several constants. Succeeding portions of the paper deal with the growth of the entire central nervous system and with the comparison of the growth of the brain in the rat and in man. Limitations of space forbid a detailed discussion here of the numerous significant results set forth in the paper. Certain points of particular interest from the biometric standpoint may, however, be touched upon briefly. First with regard to the correlation data, Donaldson finds that the weight of the brain in the white rat is very closely correlated with body weight, the coefficient of correlation between these two variables being .76 ± .01. This appears to indicate a very much closer relationship in this organism than in man, though of course it must always be remembered that the body weight data for man which have been available for the study of this correlation are autopsy records and therefore not too trustworthy. The correlation found between brain weight and age is also very much higher than the corresponding correlation in the case of man, the coefficient here being .52 ± .03. The spinal cord weight is found to be even more closely correlated with body weight than is brain weight; the coefficient being .86 ± .01. Here there are no human data available for comparison. The data presented also indicate a very high degree of correlation between the weight of the brain and the weight of the spinal cord. The coefficient of correlation here is .88 ± .01. All of these correlation coefficients are positive.

The high values of these correlation coefficients for the rat as compared with man suggest an interesting question: Are we to conclude on the basis of these results (and similar ones obtained by Kellicott from his study of the toad) that there is a general tendency for the various parts of the body to be more closely correlated in lower organisms than in man? In the writer's opinion such a conclusion is at present very doubtful for two reasons. In the first place the human data on which correlation studies have been made are meager and, from their method of collection, not altogether trustworthy. In the second place the coefficients of correlation published by Donaldson (the same considerations hold with reference to Kellicott's toad data, though not to so great a degree relatively) probably have spuriously high values. This arises from the fact that they are deduced from material which is very heterogeneous in respect to age. The biometric constants for all characters which change with age by growth will have their values affected in such material. It is a well-known fact, of which the mathematical proof was first given by Pearson, that heterogeneity of material operates to increase apparent correlation. To such an extent may this occur that several sets of data, each of which taken alone shows no correlation whatever between two characters, may when combined exhibit a high degree of correlation between these characters. Such correlation obviously has little, if any, biological significance. In the work here under discussion no account is taken of the possible effect in increasing apparent correlation of the age (= growth) heterogeneity of the material. It seems desirable if brain-weight (or other) correlations are to have full significance for comparative purposes, that they be based either on adult material in which all growth changes have reached a minimum, or at least on material which is homogeneous in respect to some definitely marked period of the life cycle.

A further point of less practical significance which is apparently overlooked by Donaldson in his discussion of correlation results is that in at least all of his cases in which the regression lines are plotted in the paper the correlation is markedly skew. In such cases, of course, the correlation coefficient can not be taken as the true measure of the actual correlation. Instead, resort must be had to the correlation ratio (η).

The facts regarding the sex relations in the weight of the central nervous system and its growth in the rat are very interesting. Just as in man the brain of the male rat is absolutely somewhat heavier than is that of the female rat of the same body weight. The difference, however, is very small. It is believed by Donaldson that this small difference which remains in favor of the male in respect to brain weight is probably open to further reduction as other variables are taken into account. In general the quantitative relations of the growth of the central nervous system are found to be similar in man and the white rat.

A paper with very much the same general standpoint as the one just discussed has recently been published by Kellicott.[3] The immediate problem with which this paper has to do is stated in the following words (p. 319):

> We are led to inquire whether the normal growth of an animal may not be actually a complex of growth cycles of component parts. It is quite possible to examine this question from the morphological as well as from the physiological side and the present paper represents an attempt to discover whether the brain and viscera of the dogfish grow similarly or in diverse ways as somewhat independent units of growth.

The investigation is based on data obtained from a series of 315 dogfish (176 females, 139 males) including specimens from birth up to those of large size and presumably considerable age (maximum weight observed 8,434 grams). On these fish the weights of the following organs were determined: brain, heart, rectal gland, pancreas, spleen, liver and gonad. In addition the total body weight was determined in each case. The weighings were made in all cases except body weight to hundredths of a gram.

Since it was impossible to determine the exact age of the specimens, the whole of the material is dealt with from the standpoint of total body weight as a base. The author justifies this procedure on two grounds, one necessity, and the other that such factors as "food and temperature are known to be of more importance than age in determining the size of fish."

The specimens studied do not represent a random sample of a fish population, but were especially selected to get a representative size series. The data so obtained were plotted, each individual being entered separately. Smooth curves were then derived from these and the bulk of the paper is occupied with a discussion of the facts brought out by these smoothed curves. The only statement as to how the observational data were smoothed is the following (pp. 322–323):

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[3] Kellicott, W. E. The Growth of the Brain and the Viscera in the Smooth Dogfish (*Mustelus canis* Mitchell). *American Journal of Anatomy*, Vol. VIII, pp. 319–353, plates 1–7, 1908.

> The curves were derived from these records by calculating a series of average weights of each organ in successive groups of individuals and a line formed by connecting these averages was then smoothed to a curve so as to reduce to a minimum the plus and minus deviations of the averages. The groups from which the averages were derived varied in extent from 100 to 1,000 grams in different regions of the entire group, according to the rate at which the character of the curve was changing.

From this the inference would appear to be that the smoothing was done by a free-hand graphical process. If this inference is correct the logical justification of the procedure in the present case is difficult to see. If the curves have enough intrinsic significance to warrant smoothing at all (as these for the dogfish certainly do) it is hard to understand why the smoothing should not be done by an accurate method. Of course cases will arise in practical work where the points to be brought out by data are not of sufficient importance to warrant the labor involved in graduating them accurately. But in the present case the whole discussion centers about the forms and relationships of the smoothed curves.

The point involved here is not a trivial or insignificant one. Any one who has had experience in fitting parabolas and similar curves to observational data knows what unexpected effects on the general contour of a curve a few outlying points may have, when the rigidly " fair " method of least squares is used in the smoothing. The difficulty may be put in this way: in curves of the sort dealt with by Kellicott the observations are very scattering over a considerable part of the total range of the curve. In many instances only two or three observations will be averaged to get a point on the smooth curve. But surely it can not be maintained that the average given by two individuals will uniformly be the same as would appear if 25 individuals were to be used. The two individuals may happen to be the mediocre ones which will give nearly the true average; but on the other hand they are not unlikely (as the right-hand ends of all of Kellicott's curves except that for brain weight clearly show) to be widely divergent from mediocrity. But the "free-hand" method of smoothing assumes, actually if not intentionally, that the average based on two individuals is just as "right" (*i. e.*, expressive of the true relationship which it is the purpose of the investigation to discover) as is that based on 25. This obvious error any adequate method of curve fitting will avoid.

The bearing of these remarks further appears clearly in the case of some of the relative curves, wherein the percentage which the particular organ weight is of the total body weight is plotted. As was to be expected from what is known of growth in man the general trend of these percentage curves is downward. While this is the general trend, several of the curves (*e. g.*, the heart curve, plate 2) show at the very beginning a slight rise to a maximum and then the downward curve. It is plain from the discussion that Kellicott considers the rise at the beginning of these percentage curves to be a real and significant phenomenon of growth. It is very doubtful, however, whether the data warrant such a conclusion. Before accepting it one would like to see the measurements of a much larger number of very young (*i. e.*, just hatched) individuals added to the curves, and then have a curve fitted by some adequate method to the observations.

The general result of this interesting and careful piece of work is to show that the regression line of organ weight on body weight in dogfish of different sizes is not of the some form for all organs. Some organs (*e. g.*, rectal gland, pancreas) show a nearly linear increase in weight as the body increases in size; others (*e. g.*, the brain) show the logarithmic like curve which one associates with growth curves. The author gives an interesting discussion of the significance of the fact that the muscular and skeletal tissues tend to "outgrow" their visceral accompaniments in forms of indeterminate growth like the dogfish. He regards the condition of determinate growth seen in higher vertebrates as "an adaptation on the part of the organism, such that muscles and supporting tissues cease their growth at such a point that brain and viscera remain competent to maintain a physiological balance."

In passing it may be noted that Kellicott's work, while itself strictly morphological, suggests on every page problems for experimental work on the physiology of the growth process. In this connection a recent paper by Burnett[4] is of interest. This paper, though not specifically concerned with growth problems as such, brings out in a very clear way the marked differential effect which may be produced on a single organ system (the skeleton) by differences in the food of the growing animal. Different foods, with all other conditions constant, led to an average difference in the breaking strength for five bones of the body of 356 pounds per 100 pounds body weight. The relative magnitude of this difference is indicated by the fact that the *maximum* observed average breaking strength was 681 pounds per 100 pounds of body weight. This difference is brought about not by an increase in the size of the bone as a whole, but by a thickening of its walls. Burnett's detailed results are well worth careful study from the standpoint of experimental morphology.

A new and suggestive view in regard to the ultimate physiology of the growth process has been put forth recently in two papers by Robertson.[5] In brief this view is stated by the author in the following words (first paper, p. 612):

> 1. In any particular cycle of growth of an organism or of a particular tissue or organ of an organism the maximum increase in volume or in weight in a unit of time occurs when the total growth due to the cycle is half completed.
>
> 2. Any particular cycle of growth obeys the formula $\log x/(A-x) = K(t-t_1)$ where x is the amount (in weight or volume) of growth which has been attained at time t, A is the total amount of growth attained during the cycle, K is a constant and t_1 is the time at which growth is half completed.
>
> 3. The above relations are such as would be expected to hold good were growth the expression of an autocatalytic chemical reaction. As I have pointed out in the introduction, cell-division has been shown by Loeb to be the expression of an autocatalytic synthesis of nuclear material. The fact that the above relations hold good shows that, in all probability, cell-growth, or the synthesis of cytoplasm, is also an autocatalytic reaction.

These conclusions, if well founded, are certainly of very fundamental importance. It therefore seems desirable to examine with some care the nature of the reasoning and the evidence on which the conclusions rest. The first point in this regard to be noted is the fundamental assumption made by the theory that the growth process is in its quantitative relations *determinate* either as a whole, or in its cyclical units. In the fundamental formula quoted A is the total amount of growth attained in the cycle. This means that unless there is assumed to be an indefinitely large number of cycles of growth, there comes a time for every organism to which the theory is to be applied, after which no more growth occurs. Regarding this fundamental assumption of Robertson's theory Kellicott, whose own researches particularly well fit him to speak with authority on the point, has the following to say (*loc. cit.*, p. 342):

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[4] Burnett, E. A. The Effect of Food on the Breaking Strength of Bones. Bulletin 107, Nebr. Expt. Stat., pp. 11–39, 1908.

[5] Robertson, T. B. On the Normal Rate of Growth of an Individual and its Biochemical Significance. *Arch. f. Entwicklungsmech.*, Bd. 25, pp. 581–614, 1908. Further Remarks on the Normal Rate of Growth of an Individual and its Biochemical Significance. *Ibid.*, Bd. 26, pp. 108–118, 1908.

> Observations of many of the lower vertebrates in nature (Fulton, '01, '06) and in captivity, such as the giant salamander and some reptiles, show that these grow indeterminately; Agassiz's, '57, observations upon Chrysemys are typical. As a recent example of the failure to make this distinction we might mention the work of Robertson, '08, who has devised certain formulæ for the description of growth and has brought out the very suggestive fact that the growth curve of an organism or organ or tissue is similar to that given by an autocatalytic reaction. These formulæ hold good upon the assumption that the organism or organ has a definite period of growth at the end of which increase in size ceases. This is true for the higher vertebrates, but for all the indeterminately growing forms we can not determine any such "final weight" of the body or organ upon which to base a formula. We could not assume the maximum discovered size as the "final weight" because this is subject to such extreme variation; in the dogfish, including both sexes, we might find the "final weight" anywhere from 2,000 to 8,000 grams and even higher.

That this point has some force in limiting the field of application of Robertson's view can not be denied. It would still appear to be possible to apply the theory to lower vertebrates and invertebrates, however, on the assumption that growth in those cases consists of an indefinite number of cycles, to each one of which separately the "law" applies. It remains to be investigated as to whether the growth in such forms is, as a matter of fact, definitely cyclical in character.

The general line of reasoning adopted by Robertson in developing his theory of growth is as follows: The starting point is the idea advanced by Loeb "that the process of synthesis of nuclein, which is the most salient phenomenon immediately succeeding fertilization, partakes of the characteristics of an autocatalyzed chemical reaction, since the velocity of the synthesis increases, during the initial stages of cell-division, in proportion as nuclear material has already been synthesized." He then raises the question as to whether the formation of fresh cytoplasm during the growth of an organism may not also be an autocatalytic reaction. It is pointed out that:

> The increase in weight or volume of an organism may not improbably be regarded as equivalent to an increase of cytoplasm, and if both of the processes concerned in growth, namely nuclear and cytoplasmic synthesis, are autocatalytic in character the increase in weight or volume of an individual with increase of time should display the quantitative relations which are characteristic of an autocatalytic chemical reaction. In the first place the temperature coefficient of growth should be that of a chemical reaction; in the second place the relation between body weight or body volume and the time which has elapsed since measurable growth began should be the same relation as that which subsists between the mass of material which has undergone a chemical change and the time in an autocatalytic reaction.

This work of Peter is cited to show that the temperature-coefficient of cell division and of growth is that of a chemical reaction. Robertson's own papers are solely concerned with the presentation of evidence to show that growth curves of organisms are of the same type as the curve of an autocatalytic reaction.

Various observations on the growth of certain animals, plants and man are cited from the literature. To each group of these data the theoretical curve of an autocatalytic reaction is fitted. Theoretical and observational curves are then compared, and it is maintained that the graduations obtained are good ones. The agreement between observation and theory which is held to be shown by these comparisons is the only new evidence presented by Robertson in support of the conclusions quoted above.

The data presented in the papers may first be considered with reference to the soundness of the contention on which the whole reasoning ultimately rests: namely, that the theoretical curve for an autocatalytic reaction actually does give a good fit for observed growth data. A careful, critical study of the data presented in the two papers indicates that in general the correspondence between theory and observation is very far from being sufficiently close to warrant the conclusion that such is the case. To bring this point out in a concrete fashion let us examine some of the tables given. Table I of the first paper (pp. 581–591) deals with Donaldson's[6] data on the growth of the male white rat in respect to body weight. In this table are given the observed body weights for rats of different ages and the calculated body weights according to the autocatalytic growth curve. In addition there is given a column showing the differences in grams between the observed and the calculated body weights. Now it is a first principle of scientific curve fitting (and on this point science and common sense are as usual in agreement) that a curve which gives a good graduation of observational data will fairly and equably distribute the errors. That is to say, a theoretical curve if it is to be regarded as fitting the data should strike through the observations in such way that there will be on the average as many and as great differences where theory is in excess of observation as there are where it is in defect of observation. If a great majority of the differences between theory and observation are in one direction there is clearly a bias and the theoretical curve can not fairly be said to be an adequate representation of the observations. Now, let us examine the actual facts for Robertson's Table I. In this table are included 63 separate observations or ordinates. In one case out of the 63 the theoretical and the observed ordinate exactly agree. Of the remaining 62 cases where theory and observation can be compared the calculated ordinate is greater than the observed in only 14. The observed ordinate is greater than the calculated in 48 cases out of the 62. Furthermore, the total deviation between observation and theory when theory is greater than observation is 19.6, whereas when observation is greater than theory the total deviation is 706.0! This certainly does not look like a fair distribution of the errors when in 77 per cent. of the ordinates the theoretical curve lies always on the same side of the observational line.

Let us turn to Table II. This table is exactly like Table I except that it deals with Donaldson's data for the growth of female white rats in body weight, whereas Table I deals with the males. In this table there are in all 50 ordinates. Of these one again shows an exact agreement between observation and theory. In 43 or 88 per cent. of the remaining 49 ordinates the calculated value is less than the observed. Only in 6 cases is the calculated value greater than the observed! Practically the whole of the theoretical curve in this case lies on the same side of the observed line. The sum total of the plus deviations equals only 1.15, whereas the sum total of the minus deviations equals 199.05!

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[6] Boas Memorial Volume, New York, 1906.

Let us take still another example, this time from near the end of the first paper. In Table IX (p. 610) are presented Donaldson's[7] data regarding the growth of the brain in the frog and the fitted curve. In this table are 21 ordinates available for the comparison of theoretical curve and observational data. The deviation between theory and observation is plus in 18 out of the 21 cases and minus in 3 cases only. Two ordinates (making with the 21 the total of 23 tabled) show exact agreement between theory and observation. In spite of this extraordinarily uneven and biased distribution of the errors this statement follows Table IX:

> It is evident that the agreement between theory and observation is excellent, such divergences as exist being evidently irregular and accidental in their nature.

Surely a system of errors in which 86 per cent. are in excess and only 14 per cent. are in defect and in which the *mean* percentage deviation per ordinate for the plus deviation is 8 per cent. can not fairly be said to be "irregular and accidental" in its nature.

Other examples showing the same thing might be cited from the papers. The tables which have been chosen as illustrations of the point under discussion have been taken in preference to others for two reasons; one that they were long tables, involving a fairly large number of ordinates, the other that the observational data in these tables were obtained by most careful and painstaking measuring and are absolutely trustworthy. On such data, if anywhere, a theoretical curve may fairly be expected to give good results.

To summarize this part of the discussion it may be said that the discrepancies between observation and theory are so great in amount, so biased in character and so frequent in the data presented that these data, as they stand, can not reasonably be held to afford evidence of any particular value in favor of Robertson's ingenious, suggestive and potentially very valuable hypothesis. It is possible that better values for the constants of the theoretical curves might be found and in this way better agreement between theory and observation be obtained. Until this is tried it would appear to be impossible to form any just and significant estimation, on the basis of the only *kind* of evidence which Robertson presents, namely, the comparison of curves, as to the value of his theory as a general theory of growth. On many general grounds the theory is particularly suggestive. Can not evidence of another and more convincing kind than that adduced in the present papers be brought forward in its support?

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[7] *Jour. Comp. Neurol.*, Vol. VIII, 1898.

The kind of evidence under discussion, when used for a purpose like the present one, can at best have but inferential significance; it can never be of demonstrative worth. It is based on a process of reasoning which assumes a fundamental or necessary relationship to exist between two sets of phenomena because the same curve describes the quantitative relations of both sets. A little consideration indicates that this method of reasoning certainly can not be of general application, even though we assume it to be correct in particular cases. The difficulty arises from the fact that the mathematical functions commonly used with adequate results in physical, chemical, biological and mathematical investigations are comparatively few in number. The literature of science shows nothing clearer than that the same type of curve frequently serves to describe with complete accuracy the quantitative relations of widely different natural phenomena. As a consequence any proposition to conclude that two sets of phenomena are causally or in any other way fundamentally related solely because they are described by the same type of curve is of very doubtful validity. A few examples will make clear the point here under discussion.

In a recent paper Armsby[8] shows that the rate of gain of protein per thousand pounds live weight in growing animals follows extremely closely the following curve: $g = 135/(a + 20)$, where g is gain in protein per day per 1,000 lbs. live weight and a is age in days. This curve, as his Fig. 1 clearly shows, fits the observational data at hand remarkably well. This equation is the equation of a rectangular hyperbola. But it is a well-known fact that the relation between degree of dissociation and degree of dilution in dilute solutions is given by a hyperbola. Now in so far there would appear to be exactly the same kind of logical basis for the conclusion that since the same curve describes the rate of protein gain in growing animals as describes dissociation phenomena, therefore rate of protein gain is a dissociation phenomenon, as would exist for the conclusion that growth is an autocatalytic reaction provided there were good agreement between observed and theoretical curves in the latter case. A point of difference in the two cases is that Robertson presents several curves in support of his conclusion, whereas Armsby gives but a single hyperbola. But even this difference is in some degree offset by the fact that Armsby's curve involves growth data from four different animals collected by a number of observers. But the most ardent advocate of the plan of deducing fundamental relationships from similarity of curve type would not maintain that the rate of protein gain in growing animals is in any causal or fundamental way directly related to the phenomenon of dissociation in dilute solutions.

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[8] Armsby, H. P. Feeding for Meat Production. Bureau of Animal Industry, Bulletin 108, pp. 1–89, 1908.

Let us take still another case. One of the fundamental gas laws is that the "pressure of any given mass of gas varies directly as the absolute temperature if the volume of the gas remains constant." The mathematical expression of this relation is the equation of a straight line. Now Galton, Pearson and their co-workers have shown, with a wealth of data drawn from man and other organisms, that the regression of offspring on parent in parental inheritance is a linear function. If the mean conditions of a characteristic of the offspring of each group of parents be plotted these plotted points will fall on a straight line, within the errors of random sampling. This result rests on a great mass of exact measurements. But of course no one would attempt seriously to maintain that parental inheritance and regression are phenomena of gas pressure.

The point which the writer would make is this: If there is good evidence *on other than quantitative grounds* that two sets of phenomena are qualitatively alike it is is pertinent and significant to present as additional and confirmatory evidence data tending to show that these sets of phenomena are similar in their quantitative relations. But similarity of quantitative relations between phenomena can not safely be taken as proof (or, in the absence of qualitative data sufficient alone practically to establish the point, even as particularly weighty evidence) of qualitative identity, because of the observed general lack of uniqueness in the quantitative relations of natural phenomena.

In a word the final *proof* of qualitative identity of phenomena must always in last analysis be qualitative in its nature; quantitative evidence in such cases can at best have but an inferential confirmatory bearing on the qualitative point at issue.[9]

RAYMOND PEARL.

EXPERIMENTAL ZOOLOGY

Are the Drone Eggs of the Honey-Bee Fertilized? Cuénot[1] has put to the test once more Dzierzon's famous theory in regard to the nature of the drones of the hive bee. Dzierzon, as is well known, furnished strong evidence in favor of the view that the egg that produces a drone is not fertilized. An obvious test of this view is found in crossing a virgin queen of one race by a male of another race. All of her worker offspring should be hybrids but her drone offspring should be purely maternal in character. It is said that the failure of one such experiment to give the expected results caused Dzerzon to abandon temporarily his theory. Other workers too have from time to time found that the drones in such cases sometimes show hybrid characters and this argument has been repeatedly urged against Dzierzon's theory despite the large amount of evidence of a different kind to the contrary.

Cuénot crossed a virgin female of the black or Italian bee of pure race with a " yellow bee " also of pure race. All the workers produced showed the yellow bands of the yellow parent; some 300 drones were black like the mother, two only showed a large yellow band at the top of the abdomen (recalling the more numerous yellow bands of the yellow bee), and about a dozen other males also showed some yellow bands on the abdomen. " Do those yellow bands indicate hybridization? " Such bands were never found in the males of neighboring hives. The experiment is inconclusive, Cuénot says, but it shows the necessity of examining not only the purity of the pure races but also the extent of their variation. The possibility that these few hybrid males may have arisen from eggs laid by the hybrid workers is not considered by Cuénot but until this possibility is also excluded the results can not be maintained to show the hybrid nature of the drones except in the latter sense. If the males

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[9] Cf. the discussion regarding the simple logarithmic growth curve on p. 304, *supra*.

[1] Cuénot, L. *Comp. Rend. Soc. Biol.*, LXVI, 1909.